

THE
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A CONTRIBUTION TO THE THEORY OF TONAL
CONSONANCE.¹

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On approaching the problem of tonal consonance one meets with three theories of explanation which are, in a way, classic:

First, the theory of Helmholtz which explains consonance by the identity of overtones among the two or more clangs, and dissonance by the presence of beats among the overtones.

Second, the theory of Lipps which is based upon the unconscious rhythms existing among the vibrations of the fundamentals, which as they coincide produce the effect of consonance, and as they fail to coincide, the effect of dissonance.

Third, the theory of Stumpf which centers about the principle of *fusion*; really, this is but an attempt to describe exactly the data of consonance, together with a reference, for complete explanation, to an unknown but fundamental physiological effect.

These theories are all sufficiently well known, and have been subjected to searching criticism with sufficient frequency to justify me in neglecting any very close inspection of them at this time. What I have to contribute to the theory of consonance is not a new conception, which in its acceptance must invalidate all other theories, but consists, rather, in some supplementary ideas on the subject which, so far as I have been able to discover from the somewhat meager literature at my disposal, have been either overlooked or too hurriedly discredited. I shall, therefore, attempt to show that in explaining the phenomena of consonance these considerations are of equal importance with, if

¹ Read before the Southern Society of Philosophy and Psychology, Baltimore meeting, December, 1908.

not of greater importance than, any others which have thus far been brought forward.

The theory around which my ideas center is one which has been placed in disrepute to a large degree by the critique which Stumpf has accorded it. In the *Tonpsychologie* (II., p. 208 f.) while discussing the various possible causes for tonal fusion, Stumpf raises the question: Is frequent combination the cause of fusion? He then proceeds to describe this theory as follows:

“Wenn neuere Psychologen Recht haben, tritt durch die blosse Häufigkeit des Zusammenseins zweier beliebiger Vorstellungen im individuellen Bewusstsein allmählig auch ohne jedes Ähnlichkeitsverhältnis zwischen ihnen eine Art Verschmelzung ein. Es entsteht eine ‘untrennbare Association’ und zugleich wandeln sich die so verwachsenden Vorstellungen in eine qualitativ neue einheitliche Vorstellung um (Chemie der Vorstellungen). . . .

“Und die Voraussetzung träre ja zu. Wenn wir die harmonischen Obertöne eines Klanges beispielsweise bis zum 16. Teilton (der vierten Octave des Grundtons) vorhanden denken, so findet sich das Verhältnis der Octave unter diesen 16 Tönen 9 mal (1:2, 2:4, 3:6, 4:8 u.s.f.), das der Quinte 5 mal, das der Quarte 4 mal, das der grossen Terz und Sext 3 mal, das der kleinen Terz und Sext sowie der natürlichen Septime 2 mal (bei Berücksichtigung der Teiltöne bis zum 18. auch diese 3 mal), das der grossen und kleinen Secunde 1 mal. Ausserdem wirkt im gleichen Sinne der Umstand, dass, wenn wir zunächst die Reihe lückenlos und die Klänge nur durch die Zahl der vorhandenen Obertöne verschieden denken, die höheren Obertöne nicht ohne die niederen, diese aber ohne jede vorkommen, und dass vom ersten zum zweiten Teilton eine Octave, vom zweiten zum dritten eine Quinte, dann eine Quarte, dann Terzen stattfinden; woraus folgt, dass auch in den verschiedenen Klängen das Octavenverhältnis am häufigsten vertreten ist und dann in abnehmender Anzahl die übrigen genannten Intervalle.”

Stumpf then proceeds to declare the principle *illusory*, mainly on two counts:

First, that there are no ‘inseparable associations.’ Experience teaches that any association can be broken up and replaced by another involving part of the former, provided only the new association be more frequently made than the old. This would be the case, for instance, when a student of music practices various kinds of intervals. As a result of this view one might expect the interval of the second by reason of its frequency in melody to form as fixed an associational fusion as would the octave because of its frequency in harmony.

Second, that there is no 'chemistry of ideas.' Being together in consciousness innumerable times does not necessarily involve fusion: were this the case the frequent tritonal chord would tend more and more to become a unit, and one might expect that a person who heard music frequently should be less capable of differentiating an octave and a fifth than one who heard music seldom.

These criticisms, though based upon the peculiarly unique character which Stumpf attributes to *fusion*, and not upon the somewhat simpler conception which I would apply to *consonance*,¹ are nevertheless well-nigh irrefutable so long as we regard the phenomena of consonance to be the result of an individual's personal experience with tonal combinations. But the whole matter takes on quite a different aspect when we envisage it from the point of view of racial experience. If we must admit that 'inseparable associations' resulting in a 'chemical' union of their constituent factors to produce a unique and unified experience, such as Stumpf regards tonal fusion to be, is *not* a common result of individual experience, we have, on the other hand, every reason for supposing that congenital associations result from racial experience of a definite and oft-recurring sort, to produce instinctive modes of adjustment and attendant complexes of experience which are fundamentally unified. Of course, such experiences are not necessarily beyond the power of a mature intellect's capacity of analysis, and I do not consider it a datum of consonance that it should or does defy such analysis.

The simple fact is that when tones are experienced they are usually accompanied by overtones. Without the capacity for analyzing these, they are still present and cause the organism to function in certain definite ways. The most intensive of the overtones are, as a rule, those which are most frequently present. They are, at the same time, those which represent the most consonant relationships: the octave, the fifth, the fourth, the major and minor third.

This view of the matter causes Stumpf's 'inseparable associations' and 'chemistry of ideas' to appear as 'straw men.' But it is fair to state that Stumpf himself, as he seeks the cause of fusion in physiological activity, rehabilitates this theory to the extent of granting that an indirect cause of fusion may be attributed to the *disposition* which the

¹There is some confusion among various authors as to the differentiation of these two terms. Stumpf tends to identify them, whereas Jodl, for instance, regards fusion as a phenomenon of sensation, consonance and dissonance as phenomena of feeling and as esthetic elements (*Lehrbuch der Psych.*, 2d ed., I., p. 362); Wundt entertains similar ideas (*Grundzüge*, 5th ed., II., pp. 111 f., 421 f.).

organism would acquire through the frequent experience of overtone intervals to respond to these as more or less unified wholes. But this disposition, it seems to me, need by no means be considered so indirect and variable as Stumpf would have it, especially when one considers it as a congenital acquisition.

The nervous correlate of a consonant relationship I regard as nothing other than a relatively simple and economic activity on the part of the sense organ and nervous system. Such dispositions, I take it, are in the nature of fundamental capacities resulting from very frequent racial experiences. Thus considered, the experience of consonance may be said to possess a uniqueness having much the same sort of quality as attaches to recognition; though recognition, of course, being based upon individual experience, is more variable.

Simple tones, though they vary in absolute vibration rate, appear again and again involving the same relationships of accompanying overtones, and therefore stimulating the nerve ends in the same functional patterns. On the other hand, more complex and irregular vibrations present ever-changing stimulations which, in the case of noises, vary to such an extent that it is with difficulty that we can memorize or reproduce the simplest of them. Dissonant relationships, when comparatively simple, though they do not have the congenital quality of easy functioning which characterizes the consonant relations, are nevertheless rather easily familiarized. In this fact we may, perhaps, find an explanation for what appears to be a gradual increase in the number of tonal relations which music makes use of.¹

As a result of racial experience, then, it seems to me that we are justified in assuming that a greater ease of functioning will naturally evolve respecting tones, which, when present in consciousness simultaneously or successively, represent relationships that the experience of overtones has made familiar. Since these relationships must exist from the very beginning of man's ability to hear tones, a congenital association may be said to form resulting in a physiological adaptation which becomes as inherent in the organism as those which account for the apprehension of rhythm, symmetry, proportion or the fusion of

¹ Müller-Freienfels, for instance, in a recent article ('Zur Theorie der ästhetischen Elementarerscheinungen,' *Vierteljahrsschrift f. wiss. Phil.*, XXXII. Jahrg., H. II., p. 204 f.) notes that the octave was regarded the most beautiful consonance by the Greeks. In the Middle Ages the fifth became most conspicuous, and only gradually was the third admitted to the rank of a consonant relation. Mozart, Beethoven and Rossini were all in their times criticized for the admission of dissonances, and to-day the music of Strauss is a bone of contention for this same reason.

taste and smell. It is not necessary to assume the inheritance of acquired characteristics to explain this disposition of the organism, because the fundamental consonant relations are present whenever overtones are stimulated.

As stated at the beginning, I do not propose to deny whatever significance may properly be construed to attach to the Helmholtzian, Lippsian or Stumpfian interpretations. There is also much to say in favor of the importance of difference tones in this connection, as has been more recently developed by Wundt¹ and Krüger.² Within the respective limitations which recognized criticism has set for them, each of these may be regarded as furnishing contributory factors of explanation. Nevertheless it should be noted that each of these theories attempts to explain the phenomena by the analysis of two or more related tones *when simultaneously experienced*. On the other hand, the theory which I support must, I think, be regarded as more fundamental than these, since it lays stress upon a physiological disposition which is acquired whenever tones are experienced, either singly or together, and refers primarily to frequency and simplicity of function rather than to the conscious or unconscious apprehension of coincident overtones, difference tones or rhythms.

In addition to these theories, however, I also recognize the immense influence on the development of consonant relations, which practical and technical exigencies, such as the character of the human voice and the musical instruments of man, have exerted. These influences I, of course, do not regard as congenital, but rather as affording an explanation for the cultivation of man's taste and interest in harmony and melody.

Turning now, briefly, to the affective and esthetic features of consonance, I am not inclined to grant to these simple relationships a necessary esthetic effectiveness, neither do I regard them properly described as elementary feelings of pleasantness. The relative simplicity and economy of their functioning lends a unique quality to the experience, but to give pleasure would require, in accordance with Professor Meyer's recently formulated theory,³ which, in the main, I accept, that the experience be enriched by the presence of contributory conscious factors. As to the esthetic character of the experience, this would obtain, according to my belief, only in case this function proved to be the central

¹ *Grundzüge*, 5th ed., II, p. 421 f.

² 'Differenztöne u. Konsonanz,' *Archiv f. d. ges. Psych.*, I., Hefte 2-3; II., Heft 1.

³ 'The Nervous Correlate of Pleasantness and Unpleasantness,' *PSYCH. REV.*, XV., 4, 5.

point in an all-pervading and relatively complete adjustment. This relatively complete adjustment finds its main characteristic in the absence of dominant volitional tendencies. It is, however, not to be considered as a *static* condition. On the contrary, it may very well denote a succession of mental states each one of which evolves from the preceding without any conspicuous demand for active volitional direction. What is often described as 'non-voluntary' or 'spontaneous' attention might be called an esthetic state were it not for the manifest absence of control. Active control is called for in the esthetic state in the sense that a certain 'set' or adaptation of the organism is requisite. But the state is nevertheless differentiated from the active *practical* consciousness of voluntary control by the relative ease and lack of effort which characterizes it. The esthetic adjustments appear as though ready-made for the percipient. He simply finds himself *en rapport* with them while all his faculties are alive to tracing out the conformable pattern which the esthetic object presents. This is the reason why the esthetic state is almost always the result of an objective stimulus, almost never composed of purely ideational factors: in the latter case the control is too indefinite.

A simple interval such as the octave is consonant by the very nature of its being. It becomes pleasant when accompanied by other subordinate experiences which contribute to it and enrich it. It is esthetic when it forces its way into the focus of attention and denotes the principal factor in a total process which signifies an immediate and relatively complete adjustment.

A consonant relationship, then, is neither necessarily pleasant nor esthetic. It may be both or neither; it may be pleasant and unesthetic, or esthetic and unpleasant. This view results from regarding consonance as the mere consciousness of a relatively simple functional disposition resulting from a fundamental characteristic of tonal stimuli. As such it may be experienced within or without the focus of attention. If it becomes pleasantly or unpleasantly toned, this is because it has become enriched or depleted through coalition with other conscious factors which either augment or interfere with its function. Here again it may be either a subsidiary or a dominant experience. The condition for its esthetic effect is that it shall become a central feature in consciousness, and as such form the dominant element in a pattern of experience which pervades consciousness as a whole and is characterized by the absence of conspicuous motives for readjustment.

The esthetic experience is enriched in the usual way when the contributory factors present tend actively to augment the central factor.

Otherwise, a state of equilibrium may obtain between the various factors resulting in an esthetic state which is affectionally neutral. Or we may even assume the possibility of an esthetic state being unpleasant when the contributory factors tend to check and deplete the central process. In case either the augmentation or depletion of the process becomes considerable — and more readily, no doubt, with depletion — the esthetic character of the situation tends to be destroyed because an emotional discharge is apt to set in. All states where an emotion is dominant are unesthetic in nature because of the fact that motives for readjustment are conspicuous. On the other hand, subordinate emotional activities may enhance the esthetic effect, and be made manifest only as a high degree of pleasure. Also, up to a certain degree, unpleasantness, due to an emotional tendency to deplete or check the dominant experience, may merely lend a heightened interest to the situation without disrupting its esthetic character — as, for instance, in the esthetic contemplation of the tragic, the melancholic, the weird and the gruesome.

PSYCHOLOGICAL LITERATURE.

TEMPERATURE SENSES.

Untersuchungen über die Temperatursinne. SYDNEY ALRUTZ.
Zeitschrift für Psychol., XLVII., 161-202, 241-286; XLVIII.,
385-396.

This investigation was undertaken in the hope of throwing more light upon the following questions: (1) Can it be determined that a heat sensation and a 'paradoxical cold' sensation, *i. e.*, a sensation produced in a cold end-organ by warm stimuli, have the same perception interval, while that of a warmth sensation is shorter? (2) At what temperature or strength of stimulus is a heat sensation first perceived on different parts of the skin? (3) Can a heat sensation be analyzed? (4) What tone has a heat sensation in general? (5) What relation does this tone bear to the cold and warmth sensations?

From the experiments which he performed, the author found that heat sensations result from a simultaneous stimulation of the cold and warmth nerves, because the perception interval of both the heat and 'paradoxical cold' sensations is twice as great as that of the warm, and lower values are obtained for the reaction time for high temperature stimuli. For these reasons he concluded that the cold nerves must have been stimulated. The reaction time of a heat sensation varies with the temperature of the stimulus and the locality of the skin affected, which is explained by the varying thickness of the skin, the difference in number of nerve cells and the susceptibility of the cold nerves to be affected by warm stimuli, since different parts of the body are accustomed to different temperatures.

From experiments on the lips and mouth, he found that pure warmth sensations, and weak ones at that, can be obtained only on two locations, (1) on the lips and (2) in the mouth, just inside the lips. On the other parts of the mouth and cheeks, it was difficult to determine whether a warmth sensation was present, before the stimulus was sufficiently increased for a heat sensation to be perceived. For different parts of the lips and the interior of the mouth, the increase of stimulus necessary to produce a heat sensation varies, and is lowest for the lower lip. On the whole, the author agrees with other writers in the view that in the interior of the mouth the warmth sense is poorly developed while the cold sense is well developed.

By varying the temperature of the skin stimulated, it was found that with a sudden lowering of the temperature of the skin, the absolute increase of stimulus necessary to produce a heat sensation is lowered, whereas the relative rise of stimulus increases considerably. With a sudden raising of the temperature, the opposite effect is obtained. In this connection, it was also observed that the ability of the cold end-organs to perceive heat sensations increases in proportion to their own rise in temperature.

Furthermore, Alrutz concludes that heat sensation cannot be analyzed, but bears the same relation to warmth and cold sensations that the color orange bears to red and yellow, *i. e.*, a heat sensation is a fusion of both warmth and cold with the cold element affecting the intensity more than the warmth. The author varied the temperature of the stimulus on a certain spot and found that heat sensations of different intensities were obtained, the weak ones having an indifferent tone, but those of greater intensity a strong and uncomfortable tone.

In order to determine the effect of these principles on bathing, some experiments were made in warm baths. In general, the results obtained were the same as above: (1) The higher the skin temperature, the higher lies the absolute increase of stimulus necessary to produce a heat sensation, and conversely. (2) The higher the skin temperature, the lower the value of the relative increase of stimulus, and conversely. (3) The heat sensations, which are produced at a temperature of 35° C. or lower, are more or less temporary and are replaced by warmth sensations, which also shortly disappear. (4) The final conclusion is that baths in water whose temperature exceeds 36° can be regarded, according to the effect produced by them on the skin, as a combination of warm and cold baths.

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VISION.

The Intermittence of Minimal Visual Sensations. I. The Fluctuation of the Negative After-Image. C. E. FERREE. Amer. Journal of Psychol., 1908, XIX., 58-129.

In a previous paper (*Am. Jour. of Psy.*, Jan., 1906) the author tried to show (1) that the intermittence of minimal visual stimuli is a phenomenon of adaptation; (2) that the intermittence is caused by involuntary eye movement; (3) that eye movement enables restoration to take place (*a*) by reducing the time of stimulation and (*b*) by shifting the area that is stimulated.

The object of the present paper is to show how eye movement can cause fluctuation of the negative after-image, and to point out the inadequacy of other theories. In succeeding papers the author will show the relation of the negative after-image to the intermittence of minimal visual stimuli and to adaptation.

The oscillatory theory represented by Plateau, Hering, and G. E. Müller disregards eye movement altogether; while the fatigue theory, represented by Fechner, Helmholtz, and Fick and Gürber, attributes direct influence to eye movement, but the causes are either unexplained or are untenable.

Hering's four arguments, based upon experiments, against eye movement Ferree shows to be untenable. Repeating the experiments under varying conditions he finds that eye movements *do* occur where Hering failed to find them, and that where Hering found no disappearance resulting from eye movement the image was at least *dimmed*, and disappeared when the intensity of the stimulus was lessened.

Exner agrees with Hering that eye movement does not cause the after-images to disappear by affecting the visual processes. Eye movement causes distraction of perception of the after-image, and hence the disappearance. Ferree finds, however, (1) that disappearance takes place only when the eye movement is rapid, and (2) that the after-image has permanently disappeared when the eye movement is stopped. Exner cannot explain why the after-image does not become visible again after the distraction has ceased. According to Exner, the more uniform the background the more distraction, and consequently the less fluctuation. But Ferree found the results the same experimentally whether a uniform or a mottled background was used.

The author then shows by experiments the causal connection between eye movement and fluctuation. In all cases where there was increased fluctuation there was increased eye movement. In particular: (1) Fluctuation does not occur when the after-image is too large or too small. Only with mean areas was there a tendency to move the eyes. (2) The steadier the fixation the less fluctuation, and conversely. (3) There were more fluctuations with a narrow strip as stimulus than with a square. (4) There were more fluctuations when the strip was turned vertically than horizontally. (5) The same results as in (1), (3) and (4) were obtained when voluntary eye movement was tried. (6) The longer the stimulus was fixated, the greater the number of fluctuations. (7) The more sensitive the individual to disturbances when fixating, the greater the fluctuations. (8) The more practiced the individual, the less fluctuation.

The author next shows the inadequacy of the fatigue theory. Helmholtz's view, that eye movement causes fluctuation by changing the illumination, does not account for the effects of varying the area of the after-image, nor for fluctuation in parts. Fechner's view, that eye movement causes vascular and nervous changes, does not explain the effects of variation in area, fluctuation in parts, nor the effects produced by varying the form and arrangement of stimulus.

Ferree explains the fluctuation and disappearance of after-image by what he calls 'streaming.' If in a bright diffuse light one close the eyes, not tightly, but so as to admit no light, and look deep into the field of vision, something like granules may be seen to flow across. There is apparently no order to the movement till the eyes are moved, when the direction of the streaming changes to that of the eye movement. The currents move with varying speed and in different directions. The streaming is narrower and heavier in the center of the field than elsewhere. The author thinks streaming is due to granules which flow over the retina. It is not due to entropy, circulation or a tear film, for (1) it is seen in the dark as well as in the light; (2) the streams have the same color as the background; (3) if the stream is weak the after-image is dimmed, if it is strong the after-image disappears.

The relation of streaming to fluctuation is as follows: (1) Only after-images of mean size fluctuate. This is because the streaming completely obscures a very small after-image and does not sufficiently obscure it when it is very large. (2) Greater fluctuation takes place when the stimulus is a narrow strip than when a square of the same area. The after-image due to the square obscures the whole area of streaming, while that resulting from the narrow strip does not. (3) There is more fluctuation when the strip is turned vertically than when placed horizontally. The heaviest streaming is in a horizontal direction (the direction of greatest eye movement); consequently, the vertical strip is more easily obscured. (4) Streaming explains how the results produced by involuntary eye movement can be duplicated by those obtained by voluntary. (5) When an after-image passes from one part of the field to another the increase and decrease of its fluctuation is what should be expected from its position with respect to the streams.

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A Preliminary Study of Experiments on Time Relations in Binocular Vision. TIMOTHY J. STEVENSON and E. C. SANFORD. Amer. J. of Psychol., 1908, XIX., 130-137.

The study, of which this paper is a preliminary report, belongs in the same general group with studies previously made by Münsterberg, by Dvůrák, and by Exner on the gaining of stereoscopic effect by presenting the two halves of the stereogram separately, each to its particular eye.

The present problem is concerned with the time interval which may come between the presentation of the two pictures without destroying the stereoscopic relief; and, in addition, the preliminary results given in this paper show the effect of the insertion of too great an interval between the presentations.

The apparatus is an adaptation of the Wheatstone stereoscope. The diagrams were exhibited through notches in two revolving cardboard discs, which could be set in such a way as to expose the two pictures successively, each for a brief period, with any desired time interval between the exposures. Three kinds of discs were used, black, white, and medium gray, because of the effect of the brightness of the background upon the duration of after-images.

It was determined that if to the actual duration of the exposure of the first picture be added the duration of the after-image, practically no interval whatever can be inserted between the periods of excitation of the two retinas without destruction of the stereoscopic relief.

Upon two points in regard to the effect of the time interval the results were uniform. First, when the interval is too long to allow stereoscopic relief, the parts of the diagram seem to be in motion, changing rapidly from the position they occupy in one picture to that which they hold in the other. Double images are not seen. And, second, as the interval is shortened so that conditions are favorable for the proper relief, the relief does not flash out suddenly in its full amount, but grows gradually with each decrease in the interval, reaching its maximum with simultaneous exposure.

The writers suggest that the latter results furnish an argument in support of Wundt's theory of complex local signs in binocular vision. If the phenomenon can be looked upon as a reflex one, the tendency to movement ought to be roughly proportional to the time during which a very brief stimulus was operative binocularly, and these experiments show this to be the case.

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Ueber das Verhältniß der ebenmerklichen zu den übermerklichen Unterschieden auf dem Gebiet der optischen Raumwahrnehmung.

J. LAUB. Archiv f. d. gesamte Psychol., 1908, XII., 312-339.

Under the direction of Külpe and in continuation of work done by Ament, Fröbes, and Lehmann upon Fechner's claim that the just observable difference is a constant and may therefore serve as a unit of measure, Laub undertook a series of experiments in the field of space perception. He chose this field because (1) the absolute impression would play no part when the stimuli were kept within narrow limits, and (2) the time and space errors could be ruled out.

Method of Procedure.—Forty brass squares, measuring 47 mm. on a side, were procured. From the center of each was cut a disc so that a series was formed in which the area of each successive disc was $\frac{1}{100}$ smaller than that of the preceding one. The largest disc was 19.5 mm., the smallest 14.8 mm. The first part of the observer's task was to construct a series in which each disc was just noticeably different from its immediate neighbor; the second part was to find the disc R_m which represented the subjective middle between the two extreme discs of the constructed row. If liminal differences are equal, R_m should coincide with the geometrical middle M of this row.

Eight observers were used in eight ascending and descending series. The space error was eliminated by changing the relative positions of the disc.

Results.—If Z represents the number of stimuli in the series increasing by liminal steps, M the geometric middle, and R_m the subjective middle between its extremities, it was found that:

1. Z , M , and R_m are independent of ascending and descending series, *i. e.*, the time error does not influence the results.
2. Z is not the same for all observers.
3. The fact that M varies for the same observer as much as it does for different observers shows that individuality is no factor.
4. R_m is widely different from M . In all cases it is nearer the larger extremity.

From these facts the author concludes that liminal differences increase in size with the absolute value of the stimulus.

Laub shows further, first by introspective analysis and then by experiments requiring a common method of judgment, that the method of making the judgment plays no part in the results obtained.

Laub's results tally with those of Ament and of Wrinch, who worked with time perception, in showing that the Fechner unit of measure has no general validity.

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Contribution à la physiologie de la tache aveugle de Mariotte. O. POLIMANTI. *Journal de Psychol. normale et pathol.*, 1908, IV., 289-302.

This discussion of the blind spot is introduced by a somewhat detailed summary of the results of experiments on the subject, particularly those of Johansson, J. Müller, Aubert, and Ovio, who showed that the area surrounding the entrance of the optic nerve is of relative, not of absolute blindness, as had previously been stated. Ovio, moreover, claims on experimental grounds that the retina in this region reacts to color as it does on the periphery of the eye.

In the second part of the paper, the author reports his experiments. Their object was to note (1) what changes each color underwent before its disappearance into the blind spot; (2) the distance from the observer at which the disappearance took place. For this purpose, a large background and small discs, 31 mm. in diameter, were prepared in colors of black, white, violet, indigo, blue, green, yellow, orange, and red. A background with a disc of a different color, 105 mm. from the fixation point, was placed in a fixture which could be moved backward and forward along a measuring rod, until the disc entered the blind spot and was no longer seen. The author was the only observer. He took pains to employ each eye successively and to avoid fatigue. His results show that:

1. All the colors lose in saturation as they enter the blind spot, passing through ash to gray before being lost in the background.
2. Orange disappears 4.3 mm. farther away from the eye than any other color; and black, the last to disappear, vanishes at a distance of 6.2 mm. nearer the eye than the disappearing point of orange. The figures in each case are the average of ten series of each color on each background.

In concluding, the author touches upon theories that attempt to explain the phenomena observed. Polimanti attributes the alteration of color to the physiological fact that the retinal layers decrease in number as they enter the blind spot, and the complete disappearance of colored discs into the color of the background to the psychological factor that the background predominates over the small gap left by the blind spot, leaving the gap unnoticeable.

M. GERTRUDE RAND.

PRACTICE.

The Effect of Practice in the Case of a Purely Intellectual Function. E. L. THORNDIKE. Amer. J. of Psychol., 1908, XIX., 374-384.

The author reports experiments in the mental multiplication of one three-place number by another for the purpose of showing the amount of improvement, its rate, progressive change of rate, and spread.

The experiment included thirty-three individuals. The time was noted, and an example was taken up and the two numbers looked at long enough to memorize them. Then, without sight of figures or other sensory aid, the multiplication was performed mentally, and at the end the time was again noted. The subjects were allowed to compare their results with the correct answer. A single measure of efficiency was obtained by transmuting errors into time by adding to the time taken per example one tenth of itself for each error made.

The improvement shown was large. The ratio of the scores for examples 90 to 95 to the scores for examples 1 to 5, equated so as to show both speed and accuracy, varied from .14 to .70, the median being .42 with .10 as median deviation. (These ratios are given, p. 377, ll. 8-10, as whole numbers instead of hundredths, as was undoubtedly intended.)

The fact of so large an improvement in so short a time by mature and competent minds is taken by the author to show (1) that "the training which the group had had for twenty-odd years in remembering facts, resisting distractions and carrying in the mind a series of complex relationships, had left this special function of mentally multiplying a three-place number by a three-place number in a very easily improvable condition;" (2) that the components of that previous training did not exert even a very moderate influence over the particular function trained; and (3) "that this improvement of over 50 per cent. must have been closely restricted to the special function involved," for one would scarcely claim that such practice would improve all of one's mental functions by 50 per cent., or 5 per cent., or even 1 per cent.

To the reviewer, the facts seem hardly to warrant these conclusions nor the implication that we have here evidence against the so-called doctrine of formal discipline. Might not the fact that twenty-odd years of training in remembering, concentrating, attending, etc., leave some special function in a 'very easily improvable condition' be looked upon as the result of such training, enabling the mind so trained

to transfer quickly such abilities to the particular function in question? And might not such a result be looked on as a highly desirable effect of general education? As a matter of fact, this is one of the claims made for the older disciplinary training by its advocates. Such training, they say, enables the mind much more quickly and easily to take up and perfect new though not wholly unrelated functions. The real question is: Did twenty-odd years' training or did inborn nature enable these individuals to make such great improvement?

A very great range in the amount of improvement was shown among twenty-eight individuals, some showing two and one half times as great improvement as others. There was positive correlation between the amount of improvement and general intellectual achievement in the twenty-eight individuals, being roughly estimated at .4. There was apparently a zero or a slight negative correlation between improvement and strength of visual imagery.

There was considerable variation in the rate of improvement, but in general, the earlier periods of practice show the greatest gross reduction in the scores. The author attempts to derive a general law of change of rate of improvement without trying to eliminate deviations due to external and internal disturbing factors.

This is done by eliminating the total amount of change from consideration in every case by taking the differences — score for examples 1 to 10 minus score for examples 11 to 20, and so on, and dividing these by the total change, *i. e.*, score for 1 to 10 minus score for 81 to 90. The curves thus obtained for 28 subjects and combined into one curve give the one rate of change from which the individual rates of change could come with the least improbability as the result of disturbing causes. From the curve thus obtained I have calculated by a rough measurement of the coördinates of the curves that 25 per cent. of the improvement resulted from the first 17.6 per cent. of the practice, 50 per cent. of the improvement from the first 33.1 per cent. of the practice, and 75 per cent. of the improvement from the first 62 per cent. of the practice, leaving the last 25 per cent. of the improvement as the result of the last 38 per cent. of the practice. The author, however, disclaims any attempt to decide whether there is any one general law of the rate of improvement.

In studying the different influence of equal practice upon individuals it was found that high mental ability tended to go with high rate of improvement, thus showing that equal practice tended to increase rather than diminish individual differences. The author concludes with the opinion that students with greater original capacity gain as much or

more from the same training. The conclusion seems to be, the better the student, the more he is able to profit by training, and, conversely, the greater the training, the greater the differences between those of higher and lower ability.

E. E. RALL.

UNIVERSITY OF TEXAS.

Normal Performance in the Tapping Test. F. L. WELLS. Amer. J. of Psychol., 1908, XIX., 437-483.

This is an attempt to further standardize an important psychological measure, that of the maximum rate of repeated voluntary movements. The taps were made upon a telegraph key which was in circuit with a magnet recording upon smoked paper.

The subject began at a given signal and tapped at a maximum rate for thirty seconds; after a rest of two and one-half minutes another series was taken; and so on until a set of five such series had been obtained from each hand. The experiment was evaluated by counting the number of times the key was struck during each of the six five-second intervals of a series of thirty seconds.

The extremes of individual variation for ten unpracticed subjects were in initial rate approximately 2 : 1, in the gross rates for thirty seconds about 3 : 2; but it does not appear that this difference is related to general quickness, and beyond efficiency of coördination it is not known upon what normal condition it does depend.

The high gross rate in tapping does not, as has been pointed out, involve superiority in other aspects of motor speed.

Using Woodworth's 'index of right-handedness,' based on the ratio of the efficiency of the left hand to that of the right, the criterion in this case is the average number of taps, executed during five series of 30 seconds each. In the average, left hand does relatively better when it precedes than when it follows the right, which would be the case if the work with one hand had a fatiguing effect upon the speed of the other, but in the individual cases this is seen to be the product of certain coarse deviations in either direction, so that no general conclusion should be drawn. This index of right-handedness here varies between .81 and .94, and shows distinct points of individual difference.

In the right hand it is quite noticeable that the first two series are generally the poorest and that there is a well marked tendency for the later series to be faster than the earlier. To this phenomenon he applies the name of 'interserial warming up.' In the left hand it is by no means so evident that such a process exists. After the first few seconds, the rate

in the individual series progressively decreases, having, on the average, some six-sevenths as much speed during the last five seconds as in the first five, the curve following the form usual to fatigue curves. For the more thoroughly studied individuals the practice curve is everywhere gradual in ascent, not most rapid at the beginning, the opposite of what we usually expect. The practice curve of the left hand does not ascend more rapidly than the right. And variations from a parallel course seem to be due to the fact that each hand does relatively better if tested after rather than before the other. This suggests that the effects are of a general nature. Practice increases the effect of the warming-up tendency, which therefore shows itself much more in the later experiments than in the earlier. The intermissions of two weeks or less have no unfavorable effect on the practice gain beyond at first increasing fatigue sensations.

There is only a slight tendency to positive correlation in the fatigability of the different hands. The right hand is ordinarily more immune to fatigue than the left; but there are individual exceptions. Practice tends to affect the average 'index of fatigue' of the left hand more favorably than that of the right. Initial rate and fatigability are negatively correlated, a fast initial rate being usually accompanied by a high fatigue loss. Fatigue tends in all respects to decrease variability. No consistent effect on variability due specifically to 'warming-up' or practice can be traced. The mean variation in the right hand tends to be larger than that for the left hand.

The subjective condition as estimated by an individual practiced in introspection bore no traceable relationship to the gross rate; it seemed, however, that the susceptibility to fatigue was greater when the grade assigned was good than when it was poor. Subjective condition and initial rate are more closely correlated.

Practice decreases the ratio of the period during which the key is held down to that during which it is released.

H. W. CARPENTER.

UNIVERSITY OF MICHIGAN.

FATIGUE AND DRUGS.

The Influence of Alcohol and Other Drugs on Fatigue. W. H. R. RIVERS. The Croonian Lectures, 1906. London, Arnold, 1908. Pp. viii + 136.

Although primarily undertaken to demonstrate the pharmacological action of drugs on the neuromuscular apparatus, the present work of Rivers is of considerable psychological interest because it shows con-

clusively a preponderating influence of suggestion in the actions of certain drugs. In the introductory lecture the author says that in selecting such a topic one of his objects was to show that experimental psychology has a practical side and that it may be of service to medicine. That he has succeeded in establishing this fact there can be no doubt. Beyond noting the occurrence of idiosyncrasies for certain drugs the individual differences in the actions of drugs have not hitherto been carefully considered by pharmacologists. The work of Rivers clearly shows that in such a comparatively simple mechanism as the neuromuscular apparatus the divergencies of drug effect are quite marked and the differences are such as we expect in other work of a non-pharmacological but of a more strictly psychological nature.

The lectures take up the actions of the following drugs: caffeine, alcohol, tobacco, cocaine and strychnine. The method used by Rivers for testing neuromuscular fatigue is a modified form of the Mosso method. In one series of experiments three sets of fatigue curves were made, at intervals of thirty to sixty minutes, each set comprising six fatigue curves. The individual curves were made at intervals of two minutes, and the separate contractions were made every two seconds. The curves on any one day were carefully compared with those of the other days, but on each day before a drug was given one normal fatigue curve was recorded as a control. A comparison was then made between the five following curves and the normal. The amounts of work in the successive fatigue periods, the average heights of the contractions, and the numbers of contractions were used as the basis on which to estimate the action of the special drug under investigation. Three methods of estimating mental fatigue were employed: the multiplication of four numbers mentally and writing the final figure of the product; the amount and the accuracy of typewriting; fatigue of attention.

An important, and in previous work almost neglected precaution to obviate the suggestive effect of the drug was that on the normal days mixtures were given similar in taste to the drug mixtures. In carrying on the series of experiments in this way the subject did not know when the drugs were given, excepting tobacco, and the factor of suggestion was minimized. It is probably for this reason that Rivers' results do not generally confirm the results of previous investigators. From a pharmacological standpoint the differences are interesting and perhaps important, but it must be remembered that, therapeutically, the physician is aided as much, if not more, by the suggestion of drug action as by the real action of the medicine.

Caffeine was found to have a decided action upon the amount of work performed, when it was taken in doses of 0.3 to 0.5 gram. It was found to stimulate the capacity for muscular work; in one subject this increased ability continued throughout the series of experiments, in the other subject there was found an initial increase in amount, followed by a decided decrease and even a fall below the normal level. The effects on the neuromuscular apparatus can be said to be independent of suggestion, interest, etc., as has been noted above, and the differences in action on the two subjects can be explained only by assuming a difference in physiological action, in one case as an accelerator of fatigue, in the other as a supporter of the nerve-muscle. In the typewriting experiment there was an increase in the amount of work on the caffeine days, and on these days there was about the same number of errors as on the normal days. The decidedly greater effect on neuromuscular work is taken to indicate a peripheral action of the drug as well as a central action.

The effect of small doses of alcohol — 5 to 20 c.c. — reported by previous workers was not confirmed by Rivers. The absence or the slightness of differences between his results on the alcohol and on the control days and the apparently marked effects found by other investigators are explained by the author to be due to the interest aroused in the previous experiments and to the stimulation obtained from swallowing the drug. The previous work in which alcohol is reported to have decreased the amount of work is dismissed by Rivers in his conclusion that all these workers have been influenced by suggestion. Observation tends to support this conclusion, for even in individuals habituated to alcohol small doses of whisky or brandy, containing not more than 5 to 10 c.c. of absolute alcohol, are found to be very efficacious in producing sleep and other effects which cannot be due to the action of alcohol alone. In the use of such small doses the author did not find any of the immediate effect which has been so prominent a finding of others, and it should also be noted that an immediate effect was not found even with the larger doses, 40 c.c. absolute alcohol, used by him.

Larger doses of alcohol were found to be followed by an increase in the amount of work, and "there was no trace of any decrease following the increase which other workers have found, although the experiment was continued much longer than in any previous research." This result is the more noteworthy, since on the days on which the large doses of alcohol were taken the subjects experienced giddiness and other symptoms. An objection to the conclusion of Rivers, which the reviewer believes to be correct, may be made by the anti-alcoholic ex-

perimenters, viz., that Rivers was aware of the nature of the drug because of the giddiness, etc., and that this fact made him and his other subject more anxious to work harder. The effect of the alcohol on the fatigue curve was found to be on the number of the contractions and not upon the height of the contractions.

On mental work the effect of alcohol is in the direction of decrease in working capacity. This was particularly noticeable when the records for a period of eight days were examined. Throughout this period there was a falling off in the amount of work, although practice should have given an increase. The lessened ability was found to exist during the succeeding five days on which no alcohol was taken, and was further increased on two succeeding days on which further doses of alcohol were given. It should be noted that these results are not constant for all individuals, and in fact "even the large dose of 100 c.c. failed to show any effect in some persons."

No personal work is recorded in the section dealing with the effects of cocaine; strychnine effects are reported from the experiments of Jones, which show a rapid rise in the amount of work on the days when large doses were taken, with small rises in the curve when small doses were taken. A peculiarity of all the strychnine experiments is that the rise, which comes early, is followed by a reaction of decrease in amount of work.

The method of taking tobacco could not be disguised, or rather no control experiments have been reported, and any mental influence must have had full sway. Smoking was followed by a decrease in ability, although the amount of the decrease is well within the limits of normal variability.

The book closes with five appendices giving discussions of the following: form of the curve representing the course of fatigue; new apparatus for studying mental fatigue; general effects of caffeine; effects of alcohol; and the multiplication method.

On the psychological side the work gives some accurate results regarding fatigue phenomena, but the reviewer believes the main value of the book and the lectures is that there has been demonstrated the applicability and importance of psychological methods in pharmacological research. In other words, the main value of the work is that it gives a new method or a new viewpoint for the future work on the action of different medicinal agents on man. It has placed in the hands of the pharmacologist a means whereby 'the hidden nature of the objects with which he experiments' may be discovered. It makes possible the accumulation of data of practical and theoretical value by many, and

the value of the present work is not to be measured by the small amount of results and theoretic conclusions which are drawn from the experimental findings. In this connection the reviewer may be pardoned for emphasizing the point since he was prominently criticized for it on another occasion (see *PSYCHOLOGICAL REVIEW*, 1908, Vol. XV., pp. 6-7), and since he believes many philosophers and introspective psychologists may hold the same view of the matter as his critic. In all sciences, for the increase of knowledge, to attain something more than a mere accumulation of discrete facts, ways of looking at the facts are necessary, methods of discovering them are essential and plans required. Given a method or a plan or a viewpoint, many are able to collect facts with some definite purpose for the understanding of the intimate structure of nerve cells, for example; or for the understanding of the world, in a pragmatic way; or for a dynamic interpretation of mental processes. This method, not necessarily the individual piece of apparatus, is what is needed in psychology as in other sciences, so that we may "look beneath the superficial, in search of the more fundamental."

SHEPHERD IVORY FRANZ.

GEORGE WASHINGTON UNIVERSITY.

SLEEP AND HYPNOSIS.

An Experimental Study of Sleep. BORIS SIDIS. *J. of Abnormal Psychol.*, III., 1-32, 63-96, 170-199.

Sleep and hypnosis can both be produced by monotony, by limitation of voluntary movements, limitation of the field of consciousness and inhibition. In hypnosis there is fixation of the attention and suggestibility, and an increased facility of reactions to external stimulations. In sleep there is relaxation of the attention, absence of suggestibility and almost complete suppression of the more complex reactions associated with mental processes.

Sidis, by the method which he terms hypnoidization, induces states which are allied to sleep on the one hand, and to hypnosis on the other. He thus utilizes conditions favorable to both sleep and hypnosis, with the exception that the subject is asked to fixate his attention instead of relaxing it. After a time the patient's pulse and respiration are somewhat lowered. He is often cataleptic and is in an unstable condition that falls now into sleep, then into wakefulness and again into hypnosis. These facts were the basis of experiments made on frogs, guinea pigs, cats, dogs, children and adults. The same methods were used with all of the animals. The condition induced in frogs was not typical sleep as observed in human beings.

Before the frogs fell into a state of prolonged passiveness they went through an intermediate stage which was highly unstable and varied from catalepsy to lethargy, and again from passivity to activity. The general characteristics of the condition corresponded to those of the hypnoidal state in man. A hypnoidal state that more and more closely resembled the phenomena observed in man, as the animal advanced in the scale, was found to precede and succeed the sleep of guinea pigs, cats, dogs and children.

Sidis believes that the hypnoidal is the primitive rest state out of which sleep and hypnosis have developed. Real sleep would too often prove fatal to the lower animals. They must be ready to jump and run or feign death through catalepsy at a moment's notice.

In developing an interpretation of these phenomena the author goes back to Weber's law and says that as the cells respond with less and less energy to stimulation the threshold rises and the stimulus falls out of consciousness. Although the cell may yet have a great deal of energy, that particular stimulus cannot call it forth. In other words, the cell is asleep as regards that stimulus. The periods of response to stimuli are waking states. It is readily seen, therefore, why monotony is essential to producing sleep. In hypnosis, stimuli are responded to with greater facility and the state is one of dissociation. In the hypnoidal state there is a redistribution of energy. If the result of this redistribution is a readiness to respond to stimuli and to further deplete the store of cellular energy, the state is hypnosis, but if the new condition allows no response to stimuli and brings about restoration of cellular energy, it is sleep.

ALICE PICKEL.

WASHINGTON UNIVERSITY, ST. LOUIS.

SUGGESTION.

The Effect of Suggestion upon the Reproduction of Triangles and Point Distances. J. CARLETON BELL, assisted by G. E. HATCH and L. T. OHR. Amer. Journal of Psychol., XIX., 504-518.

After a short sketch of work done by other investigators, our author describes the procedure and results of his experimental investigation of the problem. In the first series of experiments a set of triangles of 10 cm. base and varying in height from 49 to 100 mm. were exposed serially before observers sitting 2 m. from the object. At the moment of exposure an auditory suggestion was given, *e. g.*, 'make high' or 'make low,' and the observer was asked to make no conscious opposition to the suggestion, but to mark by a pencil dot the position

where he thought that the apex of the triangle should be. A card was provided for this purpose on which the base line had been drawn. The series was repeated with a visual suggestion — a diamond-shaped outline, the long axis 20 cm. and the short one 4 cm. being exposed immediately before the triangle which was to be reproduced. For the 'high' suggestion it appeared with the long axis vertical and for the 'low' with the long axis in the horizontal direction. A standard for the purpose of comparison was supplied by making occasional series in which no suggestion was offered.

Six observers took part in the experiment, and judgments of each form with each triangle were made. Auditory suggestion, 'high' resulted in only two of the observers making a reproduction of the distance between the base and apex of the triangles greater than the standard. 'Suggestion low' was more effective, 5 out of 6 making a reproduction below the standard. In the case of visual suggestion, 'low' was effective five times out of the six, 'high' was not effective.

Slight modifications in the stimulus gave results which showed considerably less uniformity than in the case of the first set of experiments.

The conclusion of the author is that the most potent factor in suggestion is 'the arousing of an expectant attitude of attention.' In the present case that factor is almost eliminated, and the mere sensory stimulus which is left does not produce very constant results.

F. S. WRINCH.

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VISUAL ILLUSIONS.

The Illusion of Compared Horizontal and Vertical Lines. G. DAWES HICKS and W. H. R. RIVERS. Brit. J. of Psychol., 1908, II., 243-260.

This investigation was undertaken with the purpose of answering the question whether or not the sensations arising from movements of the eye play any part in the production of the illusion which arises when the length of a vertical line is compared with that of a horizontal line.

The commonly accepted explanation of the tendency under these conditions to overestimate the length of the vertical line has been that vertical eye movements are more difficult than horizontal because a greater number of muscles are involved. As the explanation assumes that muscular effort furnishes the criterion of the movement, the illusion seems to be the natural result.

The experiments designed to test the validity of this theory consisted in studying the effect upon the illusion of prolonged and momentary exposure of vertical and horizontal lines. The method employed was to exhibit a vertical and a horizontal line at right-angles to one another so that the figure resembled the letter L. A simple apparatus was contrived by which the length of either line could be varied at will and the time of exposure easily controlled. The momentary exposures were less than one-fiftieth of a second—short enough apparently to exclude all eye movements necessary to the estimation of the length of the lines on the muscular effort theory. Observations were made with constant horizontal and varying vertical, as well as with constant vertical and varying horizontal lines.

In the first series of observations the investigators served also as subjects, but as their knowledge of the conditions of the experiment introduced a disturbing factor, a further series was arranged in which five men unacquainted with the problem acted as subjects. This series was continued for fifteen days. In each instance the observer was asked to name the line which seemed to him the longer, or to say when he could discern the difference.

The general result of this series confirmed that of the preliminary experiments. The illusion was quite as effective when the lines were exposed momentarily as when the exposure was prolonged. But the number of positive judgments was decidedly greater with the momentary than with the prolonged exposures. With the momentary exposures the difference in the lengths of the lines was decisive, but in the prolonged it was often very difficult to come to a decision, and the uncertainty was clearly due to hesitation and oscillation of judgment. The greater definiteness of the answers with the momentary exposures seems significant, though the total number of right and wrong answers showed no difference. The illusion was, however, in each instance more definite and decided with the momentary than with the prolonged exposure. The explanation given to this fact by the investigators is, that in the former case the immediate answers were based purely on the sensory data, while with the prolonged exposures the answers were the results of the complication introduced by such factors as knowledge about the nature of the illusion, experience arising from instruction in drawing, and other like elements of knowledge. It thus appears that culture introduces somewhat of a problem here, for the average man has a fund of knowledge which necessarily influences his interpretation. It became very evident that more definite and consistent results could be obtained in observations on illusions from

people of low culture than from civilized observers. This confirms former observations made by Mr. Rivers on the inhabitants of Torres Straits.

The results of the investigation seem to be a very decided answer to the question involved, and point to the conclusion that the illusion was not due to the influence of movement factors, since the illusion is present in an equal or greater degree when no eye movements, effective for such judgments, could occur.

The objection may be made that although movements were not actually carried out in the apprehension of the momentarily exposed figure, the genesis of such illusion may have been originally due to such movement, and in the experiments the influence of previously acquired experience could not be excluded. Yet it is difficult to see how the results of the investigation can be reconciled with the hypothesis that ascribes the illusion to the influence of eye movements.

MARGARET THOMPSON.

WASHINGTON UNIVERSITY, ST. LOUIS.

The Effect of Practice on the Perception of the Müller-Lyer Illusion. E. O. LEWIS. *British Journal of Psychol.*, 1908, II., 294-306.

The aim of this investigation was to determine the amount of the illusion when the figures were exposed for prolonged and momentary periods, and to find the influence of practice in the prolonged and momentary observations.

Two plates were used for projecting the figures on a screen. The first was cut so as to form the Brentano figure, in which a straight line *AC*, bisected by an arrow head with the vertex pointing to the right, has also arrow heads at *A* and *C*, the vertices in each of these two cases pointing to the left. In this figure, as is well known, one half of the line *AC* appears longer than the other. In the second plate two separate lines of equal length have arrow heads at their extremities, the vertices in one case pointing inward and in the other outward. Light passing through the apertures of these plates was conducted through lenses and thrown upon a screen. The plates were constructed with grooves, so that the lines might be made longer or shorter, or remain equal. In the first (Brentano) figure, one portion of the line remained of fixed length while the other was varied, and in the second figure one of the two lines was made the standard by which the other was estimated. The difference in the length of the lines was mechanically measured and afterwards compared with the judgments of the subjects, whose accuracy of observation was, of course, disturbed by

the illusion caused by the oblique lines. The duration of the images flashed upon the screen was controlled so that it might be momentary or prolonged.

Four of the six subjects were acquainted with the nature of the illusion, but only the experimenter knew what the effect of practice would probably be. The investigation showed that the effect of practice upon momentary observations is too ambiguous for definite conclusions. In the case of prolonged exposures, however, practice caused the illusion to diminish and finally to disappear. The subjects stated that with practice the lines-to-be-compared became more prominent and hence the distraction caused by the oblique lines became less. Previous knowledge of the illusion did not seem to be of assistance in overcoming the illusion. The experimenter himself required more time than any of his non-informed subjects. The effect of practice upon the illusion was found to be somewhat permanent. If the second plate was reversed, thus putting the two lines in a different position with relation to each other, the illusion returned but was not so great, and was more easily overcome.

The tabulated readings show that the momentary illusion was greater than the prolonged, a fact that would seem to disprove Wundt's theory that the illusion is due to movements of the eyes. This greater sensitiveness in momentary observations indicates also that eye movements do not play so important a part as some have thought in the accurate measurement of spatial magnitudes.

The experimenter points out that, as the subjects were not aware of a change in their readings, notwithstanding the large change in the relative magnitudes of the lines, introspection cannot be relied upon in solving problems of genetic psychology.

HELEN PRITCHARD.

WASHINGTON UNIVERSITY, ST. LOUIS.

A New Visual Illusion of Direction. JAMES FRASER. British J. of Psychol., 1908, II., 307-320.

In this investigation the lines of the figures which produce the illusion "consist of a series of visibly discrete similar parts, all inclined at the same small angle to the line of direction of the series to which they belong," instead of being uninterrupted black or white lines on a contrasting background, the more usual structure of the visual illusion of direction figures. Fraser does not venture an explanation of the illusion which his numerous figures show, but he offers experimental evidence which seems to eliminate eye-movements

as a factor. This is indicated from the fact that the illusion is noticeable with light flashes of one-eighth to one-half of a second. He also found some peculiarities which differentiate it from the Zöllner illusion. For example, no definite suggestion of perspective is present, and, again, while the illusion is modified by steady fixation accompanied by peripheral attention, it is never abolished, as in the Zöllner illusion. Fraser is of the opinion that two 'visual integrative processes,' are factors in the illusion, one a confluent union, 'corresponding to a line joining the centers of areas of the units,' which becomes more evident the nearer the units of direction are to one another in serial position, and another, a process which represents collectively 'the trends or tendencies of the units' and is more noticeable the more clearly the units of direction are impressed upon the median or peripheral, rather than the central, portions of the retina.

EDGAR JAMES SWIFT.

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PLATO ON THE WILL.

Plato's Psychology in its Bearing on the Development of Will.

MARY HAY WOOD. Mind, 1908, XVII., 48-73, 193-213.

As it appears to the reviewer, this essay does not contain anything like a pure psychological discussion. Plato himself never attempted to write anything like psychology in the modern sense of the word. It seems, therefore, to have been the task of the writer to indicate the phases of psychology as found in his writings, and it is quite natural that she should not be able to expound it in modern psychological terms. In spite of the fact that it deals with psychical activity, it is rather metaphysical than psychological in its nature.

Psychical activity is discussed in four divisions: appetite, emotion, thought and reason, and will. This order proceeds from the lowest and the least developed faculty of mind, as Plato regards it, to the highest and the most developed one. While recognizing the great difficulty in a proper translation of the terms used in the discussions, the reviewer is inclined to think it somewhat improper to employ so many original terms with no attempt at translation, however inadequate.

The writer recognizes in the first place the treatment of *Will* by Plato, although he did not use that term. The relation of Plato's problem to that of Socrates, which is briefly mentioned, is followed by a discussion of Plato's conception of the essential characteristic of soul, as self-directing movement, the notion of movements as two-

fold, implying form and matter, the doctrine of changing in changeless, the self-movement as a divine creation, and its impulse as eternal. The writer regards as ill-founded the accusation that Plato divided the mind into faculties.

Desire, self-directing principle in man, is the general term for all manifestations or forms of movement; and the conception of *pleasure* is the unchecked, the limitless activity of soul. *Appetite* is regarded by Plato as the lowest, least true and least complete activity of soul, while *emotion* is the higher assimilative æsthetic activity, which works through imitation, and has receptivity and power of movement.

In discussing the growth and development of emotional elements, the writer points out Plato's emphasis upon the love of beauty. Plato says: "Mind can come to a knowledge of its own nature through sight and love of beauty, and love of beauty is the beginning of 'the heavenward pilgrimage.'" The development of reason is the development of the power of passing from many particulars of sense to the 'one universal reason.' This power of universal reason is fully developed in very few minds; all others are in a transitional state with a greater or less ascendancy of the emotional elements in mental activity. But there is no fast line between the emotional and reasoning activity.

Plato holds that the clue to reason is found in the power of abstraction, which is analogous to that of the artist, and is manifested in its most elementary form in *number*. Reason is the full and final development of the soul's activity, while truth is seen to be the unifying principle which gives form to what was otherwise formless, simplicity to what was complex, and wholeness to what was discrete. The 'idea of the good' is just this harmony and simplicity. One can grasp no reality without the power of universal reason or principle, and this reason or thought is concerned with the whole soul.

Turning to the final topic of the will, the writer shows the essential conception of Plato. Plato regarded the will as found in the *wholeness of mental activity*, and reason alone is that power of discrimination which can duly analyze the whole into the parts, and again give to each part its place in the whole. The condition of self-mastery is the result of a growth of mind, and self-mastery is self-knowledge. Desire and emotion must be subordinate to reason, and in this way the mastery of self by self, 'training and being trained' by oneself, is possible. Reason is above all necessary to any stable development of *will*.

"The will of a community," Plato says, "originates in the will

of only the few who have attained a complete development of soul." Of the starting point of the series, the actual origin, says the writer, Plato gives no account save in myths, where it is attributed to the action of *Divine will and reason*, a will whose purpose and whose existence are the perfection of *beauty, harmony, and truth*.

TODA CHO.

UNIVERSITY OF MISSOURI.

REPORTS.

THE CLARK UNIVERSITY CONFERENCE.

From July 6 to 10, at the invitation of President G. Stanley Hall, a notable Conference on Child Welfare was held at Clark University. Its object was to bring together all organizations interested in the child out of school, to correlate what had already been accomplished and to organize in order to attain better future results. Two common needs were made apparent in the various addresses, first, the need for trained workers, which need the universities were urged to provide for by establishing such departments as the new Child Institute to be opened at Clark this fall; and secondly, the need of popularizing the knowledge already possessed, so that it will be accessible to the most ignorant parents. This, it was felt, can best be done by the establishment of a Federal Bureau at Washington.

The Conference organized itself permanently under the name of the Conference on Child Welfare and Research, its object being to encourage in every way the scientific study of children and the popularizing of the results of such study. It will meet annually, and all persons interested in these objects are eligible to membership on payment of the dues of one dollar.

The list of officers is still incomplete: President, G. Stanley Hall, Clark University; Secretary, Dr. Henry S. Curtis; Executive Committee: Mrs. Frederick Nihoff, President of the Mothers' Congress, Miss Patty Dill, Columbia University, Professor Livingston Farrand, Columbia University.

AMY E. TANNER.

BOOKS RECEIVED FROM AUGUST 5 TO SEPTEMBER 5.

- Fifty Years of Darwinism.* Centennial Addresses in Honor of Charles Darwin, Before the American Association for the Advancement of Science, Baltimore, Friday, January 1, 1909. New York, Holt, 1909. Pp. iii + 274. \$2 net.
- De l'Illusion, son Mécanisme psycho-social.* ALBER. Pref. by R. MEUNIER. (Biblioth. de psychol. expér.) Paris, Bloud et Cie, 1909. Pp. 119.
- La rééducation physique et psychique.* Dr. LAVRAND. (Biblioth. de psychol. expér.) Paris, Bloud, 1909. Pp. 121.
- Le Problème de l'Action: La Pratique Morale.* GUSTAVE RODRIGUE. Paris, Alcan, 1909. Pp. iv + 203. 3 fr. 75.
- Berkeley and Spiritual Realism.* ALEXANDER CAMPBELL FRASER. London, Constable & Co., 1908. Pp. xi + 85. 1/ net.
- Le Régime des Aliénés.* FERNAND DUBIEF. Preface by Dr. BAJENOFF. Paris, Jules Roussel, 1909. Pp. 350. 3 fr. 50.
- A Theory of the Genetic Basis of Appeal in Literature.* HOMER CLYDE HOUSE, Ph.D. (Dissertation; University of Montana.) Lincoln, State Printing Co. Pp. 77.
- Letters, Lectures, and Addresses of Charles Edward Garman.* Edited by ELIZA M. GARMAN. Boston and New York, Houghton, Mifflin, 1909. Pp. xiii + 616. \$3 net.

NOTES AND NEWS.

THE seventh International Congress for Psychology will be held in 1913, in the United States, the city to be determined later by the committee in charge. The following officers have been appointed: Honorary President, William James; President, J. Mark Baldwin; Vice-Presidents, E. B. Titchener, J. McK. Cattell; General Secretary, J. B. Watson. A report of the sixth Congress, held in Geneva last month, will appear in an early number of the BULLETIN.

THE Second Decennial Celebration of Clark University was held during the week of September 6 to 11. The department of Psychology celebrated the event by a series of lectures and conferences, the former by a number of eminent scientists, American and foreign.

PROFESSOR IRVING KING, of the University of Michigan, has been made assistant professor of education in the State University of Iowa.

PROFESSOR J. MARK BALDWIN has been made Docteur ès science, *causa honoris*, of the Geneva University.

MR. H. C. McCOMAS, A.M., has been appointed demonstrator in psychology at Princeton University.

THE following are taken from the press :

PROFESSOR G. H. HOWISON has been made professor emeritus of philosophy at the University of California.

DR. RAYMOND H. STETSON, of Beloit College, has been appointed professor of psychology at Oberlin College.

DR. FRANK N. FREEMAN, for the past year traveling fellow in philosophy and psychology of Yale University, has been appointed instructor in educational psychology in the University of Chicago.